Quantisation, Collapses and the Measurement Problem

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Abstract

The present paper contains a new attack on the measurement problem. The point of departure is a realist view according to which i) state functions in quantum theory describe physical states of affairs and not information states attributed to observers, and ii) in theses states, some observables are indeterminate and not merely unknown, i.e., value determinism is rejected. Furthermore, quantisation of interaction is accepted as an empirically established fact, independently of any interpretations of quantum theory. From these assumptions it follows that Hermitian operators replacing classical variables may be viewed as representing actions from the environment done on physical systems represented by the state functions upon which the operators operate. Sometimes this influence is followed by a discontinuous, indeterministic and irreversible state change; in other words, the system undergoes a collapse, which is represented by a projection operator.

Thus, assuming a realistic view on quantum states and their changes, we have an explanation for the collapse of the wave function. Since the collapse is a discontinuous, random and irreversible state change, the classical form of physical explanation in terms of a mechanism which describes how a system continuously changes its state is impossible. Hence, if we accept quantisation of interaction, we must give up our demand for a ordinary mechanical explanation for the collapse. Neither can we state, in advance, sufficient conditions for the collapse, since it is an indeterministic theory.

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1 Introduction

The measurement problem has occupied physicists and philosophers ever since von Neumann published his seminal work *Mathematische Grundlagen der Quantenmechanik* in 1932. In this book von Neumann introduced a distinction between two kinds of measurements, later labelled, by Pauli, measurements of the first and of the second kind. Characteristic of a measurement of the second kind is that the value of the measured variable can be predicted with certainty. (Theoretically, this value is an eigenvalue to a Hermitian operator when operating on the state function representing the state of the measured system before the interaction takes place.) If the outcome cannot be thus predicted, it is a measurement of the first kind. In such cases the state Ψ of the object before measurement can be described as a superposition of eigenstates ϕ_i to a Hermitian operator $\mathbf{O}$, chosen so as to correspond, in a certain sense, to the measured observable. Thus we may write

$$\Psi = \sum_{i=1}^{n} c_i \phi_i$$

where the set $\{\phi_i\}$ is a complete set of orthonormal eigenstates to $\mathbf{O}$ and the coefficients c_i are real or complex numbers, which, when squared, give us the probabilities for the different possible outcomes. The measurement result is one of the eigenvalues to the operator $\mathbf{O}$, corresponding to one of the eigenstates. Thus, immediately after the measurement the system is in such an eigenstate. Hence, a measurement of the first kind is associated with a reduction of the superposition to one of its components:

$$\Psi = \sum_{i=1}^{n} c_i \phi_i \rightarrow \phi_k \qquad \text{for some k} \tag{1}$$

This change is also referred to as a *collapse* of the wave function. Such a collapse is a discontinuous, indeterministic and irreversible state change.

It is discontinuous since there exists no continuous time evolution operator that destroys all but one of the components in the superposition. Any continuous evolution operator $\mathbf{U}(t) = exp(-i\mathbf{H}t)$, where $\mathbf{H}$ is the Hamiltonian, is unitary and linear. (I have omitted Planck's constant here.) Linearity means that

$$\mathbf{U}(t)\Psi = \mathbf{U}(t) \sum_{i=1}^{n} c_i \phi_i = \sum_{i=1}^{n} \mathbf{U}(t) c_i \phi_i \tag{2}$$

It is well known that $\mathbf{U}$ is unitary iff $\mathbf{H}$ is Hermitian. McLaurin expansion gives us $exp(-it\mathbf{H}) = 1 - i\mathbf{H}t + (i\mathbf{H}t)^2/2! - ......$ Hence, a unitary evolution cannot give the result $\mathbf{U}\phi_i \equiv 0$ for any of the states ϕ_i, which means that all components in the superposition will survive the unitary and linear evolution. (At a particular point in time only one component may be distinct from zero, but time goes on and at later moments several components have non-zero amplitudes. A time evolution operator cannot transform a superposition state to a state where all but one of the components of the superposition have a constant zero amplitude *for all times after a certain time t*). It follows that, since Ψ is not an eigenstate to the operator **O**, nor is $\mathbf{U}\Psi$; hence no continuous evolution operator can transform Ψ to an eigenstate to the operator **O** unless Ψ already is an eigenstate to that operator. Hence, the change (1) cannot be continuous.[1]

The collapse is also indeterministic; The original state function Ψ does not contain any information about which term will survive the collapse in a particular case. We only have a probability distribution over the set of possibilities. Hence we cannot predict which possibility will realise in a particular case. This might be due either to fundamental indeterminism in nature or to the fact that the state function Ψ is an incomplete description of the state. Many philosophers and physicists, Einstein for example, have assumed that quantum mechanics is incomplete and that nature is deterministic. But all efforts to show that quantum mechanics is an incomplete theory have been in vain, and there is no positive evidence that quantum mechanics is wrong. Hence there is strong reason to assume its completeness as a point of departure in philosophical discussion of quantum theory. If quantum mechanics is complete, the collapse is discontinuous and indeterministic. and the present author aims at an *interpretation* of our present theory, not to improve it or replace it with a better one.

Finally, since we can apply the same argument for the reversed state change $\phi_k \rightarrow \Psi$, it follows that (1) is irreversible in the sense that it is impossible to arrange conditions such that with certainty we can force the system back to its former state.

By contrast, if a state change can be described by a unitary operator $\mathbf{U}(t) = exp(-i\mathbf{H}t)$ then the change is continuous, deterministic and reversible.

There are also two other discontinuous changes in quantum theory, space inversion and time reversal.These changes, however, are deterministic and reversible, so it seems that not all discontinuous changes are indeterministic and irreversible. But that is a mistaken conclusion; time reversal and space inversion are no real physical state changes, but rather changes of mathematical representations of the one and the same state. In order to become more clear about this issue we need to discuss

[1]Adherents of the decoherence interpretation claim that we may look upon the reduction of the superposition as an approximation and that in reality there are time evolution operators that very fast reduces the amplitudes of all but one of the terms to almost zero.The decoherence view simply takes for granted that *all* time evolution is represented by unitary operators. I think we have no good reasons to believe that.

the question when two state descriptions are descriptions of *the same state*.

2 Individuation of states

Let us assume that '$\Psi(x,t)$' (where x represent all spatial coordinates) is a description of the state of a particular physical system. One may now ask which expressions '$\Phi(x,t)$' are descriptions of the *same* state of this particular system? It seems to me that two state descriptions connected by a unitary or antinunitary operator in a certain sense are descriptions of the same state of this physical system. This is not the common view; it is usually said that different rays in Hilbert space represent different states, and different rays may be connected by unitary transformations. This difference reflects different ways of individuating states, which may be called respectively *kinematical states* and *dynamical states*. Different rays in Hilbert space represent different kinematical states (and if ϕ_i and ϕ_j belong to the same ray, they are identical kinematical states) , whereas if two state descriptions are connected by a unitary or anti-unitary transformation they are descriptions of the same dynamical state. The reason for introducing a somewhat non-orthodox way of individuating quantum states is that it is connected to interactions whereby conserved quantities are exchanged, which is the crucial feature of collapses. Here is my argument for introducing the notion of dynamical state.

Consider two observers looking at the same system at the same time.[2] One observer sees the system being in a state represented by the ray R while the other observer sees the system being in a state R'. (So far no assumption about identity is made.) The system may undergo a transition to another state belonging to a set of states $\{R_i\}$ (as described by the first observer) or $\{R'_i\}$ (as described by the second observer). Obviously, the two observers must find the same transition probabilities

$$P(R \to R_i) = P(R' \to R'_i) \tag{3}$$

By assuming that transitions probabilities are invariant under a transformation from one observer's point of view to the other, we may conclude that there is an unitary or anti-unitary operator U on the relevant Hilbert space that preserves these transition probabilities. This is what we learn from Wigner's theorem (Wigner, 1931; Weinberg, 1995):

Wigner's theorem: For any transformation $R \to R'$ that preserve the transition probabilities, we can define an operator U on the relevant Hilbert space such that if Ψ is in a ray R, then $U\Psi$ is in the ray R' and U fulfills either

$$\langle U\Psi, U\Phi\rangle = \langle \Psi, \Phi\rangle \tag{4}$$

[2]This argument is adapted from Weinberg (1995, 91)

$$U(\xi\Phi + \eta\Psi) = \xi U\Phi + \eta U\Psi \tag{5}$$

or else

$$\langle U\Psi, U\Phi\rangle = \langle\Psi, \Phi\rangle^* \tag{6}$$

$$U(\xi\Phi + \eta\Psi) = \xi * U\Phi + \eta^* U\Psi \tag{7}$$

Equations (3) and (5) are the unitary and anti-unitary conditions respectively. The well-known unitary condition, $U^\dagger = U^{-1}$ follows from equation (3). Because the adjoint operator $A^\dagger$ to an anti-linear operator A is defined by

$$\langle\Phi, A^\dagger\Psi\rangle \equiv \langle A\Phi, \Psi\rangle^* = \langle\Psi, A\Phi\rangle \tag{8}$$

it follows that the conditions both for unitary and anti-unitary transformations take the form $U^\dagger = U^{-1}$.

Together with the principle that transformations between frames of reference preserve transition probabilities, we have that any such transformation are represented by either a unitary or an anti-unitary operator.

Unitary transformations are all continuous transformations as can be seen from the following argument:

The identity transformation $R \rightarrow R$ represented by the operator *U=1* is, of course, unitary and linear. It follows that any transformation that can be changed into the identity transformation by a continuous change of some parameter must also be unitary and linear rather than anti-unitary and anti-linear. Hence, rotations, translations in space, translations in time and boosts, which all have the identity transformation as the limit when the relevant parameter approaches zero, are all unitary and linear.

By contrast, anti-unitary transformations are discontinuous. The two anti-unitary transformations of interest are change of time direction and space inversion.

The change of time direction in our representation of events has nothing to do with the real time ordering of events; it is rather the result of a change from representing the temporal relation '... after ...', by the mathematical relation '$\ldots > \ldots$' to its converse '$\ldots < \ldots$.' , when events are attributed time coordinates. In practice, of course, we mostly choose the first alternative of representing later times by bigger numbers. Since this is a mere convention, time reversal is a mere change of convention, of choice of parameterisation, which of course cannot change any probability distributions.

Space inversion, represented by the parity operator in quantum mechanics, may at first sight pose a problem, since parity is not conserved in weak interactions where W or Z bosons are exchanged; there is a left-right asymmetry in those interactions. However, this occurs only in *interactions* between two systems; this does not contradict the thesis that a parity operator representing a change of coordinate system does not change any probability distributions.

A weak interaction is a quantum interaction whereby the states of the interacting systems undergo irreversible state changes. The wave function for a system considered in isolation, i.e., not exchanging any conserved quantity with another system, may be represented in either of two coordinate systems which are mirrors of each other; the change from one to the other is represented by a parity operator and such a change will of course not change the probability distributions of its observables.

The crucial question is whether the converse of Wigner's theorem is true, i.e., does it hold that for every unitary or anti-unitary transformation the probability distribution of all observables are left unchanged? That this is the case is easy to see. Consider a wave function Ψ and an operator $\mathbf{O}$ having the set of eigenfunctions $\{\phi_i\}$, $i = 1, 2, ...n$. The eigenvalue corresponding to ϕ_i is c_i and the probability for the value c_i is $|\langle\phi_i, \Psi\rangle|^2$. Then let us apply a unitary transformation U on Ψ. The result is

$$U\Psi = U \sum c_i\phi_i = \sum c_i U\phi_i \tag{9}$$

because unitary operators are linear. In order to have the same probability as before we must require for all i

$$|\langle U\phi_i, U\Psi\rangle| = |\langle\phi_i, \Psi\rangle| \tag{10}$$

But this condition is easily seen to be true since it is a strait-forward consequence of the unitary condition (3). A similar argument can be given for anti-unitary operators. Hence every unitary or anti-unitary transformation preserves the transition probabilities and conversely every transformation which preserve transition probabilities is either unitary or anti-unitary.

We may now summarize: all transformations which belong to the class of continuous changes of frames of reference, viz. space translations, rotations, boosts and time translations + time reversals and parity changes are represented mathematically by operators which fulfill the condition $U^\dagger = U^{-1}$.

Furthermore, it seems indeed reasonable to say that such changes are no changes of the intrinsic state of a system, but only a change of coordinatisation used for its description. Changes of coordinatisation should not result in any change of intrinsic properties of a system, and this is indeed the case for transition probabilities under unitary or anti-unitary transformations of the state vector. All changes for which the condition $U^\dagger = U^{-1}$ is fulfilled are thus merely changes of *description* of one and the same dynamical state. We need a concept of the state of a system such that states do not change merely by change of coordinate system. So I propose the following identity criterion for dynamical states:

Identity of dynamical states: Two states Ψ and Φ are identical if and only if there is a unitary or anti-unitary transformation U such that $\Psi = U\Phi$.

What has been said is, I hope, easy to accept, except perhaps of time translations. But just as translations along the spatial axes can be interpreted as mere change of position of the observer relative to the system, a mere change of the observer in time, or what amounts to the same, a mere change of the zero point of the time axis, should be irrelevant for the physical state. The time translation operator is $exp(i\mathbf{H}/t)$, hence the transformation

$$\Psi \to exp(-i\mathbf{H}t/h)\Psi \tag{11}$$

which is a unitary transformation, represents a mere change of position along the time axis. This translation does not change the probability distributions for observables, provided the Hamiltonian is a constant of the motion, and is therefore no change of the dynamical state.

2.1 Active versus passive transformations

So far I have discussed so called passive transformations, but there are also active transformations, i.e. transformations in space and time of systems in one and the same frame of reference. Prima facie, such transformations may be viewed as real state changes. Since active and passive transformations have the same mathematical representation, they do not differ regarding transition probabilities. But then, is it reasonable to say that a passive transformation do not represent a real state change, whereas an active transformation does represent one, despite they do not differ as regards transition probabilities? It depends on purpose.

In classical physics the notion of the state of a system is used both in kinematics and dynamics, albeit in different senses. The kinematical problem is essentially the problem of keeping track of different objects, in a given coordinate system, when they move around and in order to do that we attribute trajectories in configuration space to them; a trajectory is a continuous set of occupied positions in configuration space and change of position is change of state in configuration space.

The dynamical problem, by contrast, is the proper description of interactions between physical objects and forces. A physical object remains in the same *dynamical state*, i.e., it has the same velocity, so long it does not exchange energy with its environment. Newton formulated this in his first law: a body remains in its state of motion if no forces acts on it. Newton's second law tells us that a change of momentum requires interaction with the environment, i.e., being acted upon by a force.

The distinction between kinematic and dynamic state thus cuts across states in configuration space, since velocity changes are changes of dynamical state whereas position changes are changes of kinematic state.

So if the purpose is restricted to identify an object's consecutive positions in a given frame of reference, we should distinguish between active and passive transformations as regards identity of states; passive transformations do not change the kinematical state, whereas an active transformation does. But if the purpose is to track an object's interaction with other objects, i.e., if we are interested in the identity of dynamical states, we may conclude that the distinction between active and passive transformations is irrelevant. Neither a passive, nor an active transformation changes the dynamical state.

The same conclusion may be drawn in quantum theory, provided we regard transition probabilities as defining the dynamical state of a system. And that we may reasonably do, for giving the transition probabilities for a system is giving a description of its disposition for behavior in all interactions with other systems.

The kinematical state of a quantum system may be said to be its wave function modulo the phase; just as in classical mechanics one can calculate the value of all state variables of a system from its state in configuration space, one can calculate all the probability distributions for all observables from the wave function for a system. And just as we may distinguish between active an passive transformations when talking about classical kinematical states, we must do that if we want to track the wave function's evolution in a given frame of reference. Similarly, the dynamical state of a quantum system does not change unless it interacts with other systems.

Summarizing, a change of the dynamical state of a quantum system occurs if and only if an interaction with something else, 'the environment', has taken place.

Hence, since neither unitary nor anti-unitary transformations represent any change of the *dynamical state* of a system, we may conclude the only indeterministic state changes are dynamical state changes. Hence, the identity criterion for states suggested above is the proper identity criterion for dynamical states.

We may further conclude that there is no universal identity criterion for states; to be precise we must relativize and talk about *the state relative to a descriptive framework* and different principles for state individuation serve different purposes. In what follows I am discussing only dynamical states and state changes.

3 The Copenhagen Interpretation - and some alternatives

A discontinuous, indeterministic and irreversible state change was never heard of in fundamental physics before quantum theory; for a very long time it was taken for granted that real physical changes are continuous and deterministic.

In cases where observations indicated that a discontinuous change had occurred, this discontinuity was always interpreted as the result of insufficiently fine-grained resolution of state descriptions. Similarly, unpredictable state changes were not taken to indicate indeterminism in the real world, but resulting from incomplete knowledge about initial conditions and so far neglected circumstances. (For example, the result of flipping a coin or throwing a dice is completely predictable and hence deterministic, if the initial state and the impact from the environment is known.) Furthermore, it was generally believed that physical state changes basically are reversible and that irreversible changes occur first when we aggregate to statistical descriptions of macro systems; irreversibility was the result of loss of information in aggregation. Hence, the entrenched view was for a very long time that all real changes in nature are deterministic, continuous and reversible and quantum theory appeared to contradict these views. One seemingly natural way out is to take the collapse to be a mere change of *information state* on part of the observer, not a change of *state of the object* itself. This view is natural, not to say unavoidable, if one assumes an ignorance interpretation of quantum probabilities.

Bohr and Heisenberg elaborated on this idea, which resulted in the *Copenhagen Interpretation* of quantum mechanics, a view that for decades was dominant.[3] (There are some differences between Heisenberg's and Bohr's views, but that is of minor importance for the present discussion.) This interpretation says, among other things, that measurements by their very nature *disturb* the state of the measured object in an uncontrollable way and that's why a classical, i.e., continuous, deterministic and reversible description of the measurement interaction is impossible. This view was, moreover, congruent with the general instrumentalist view, once prevalent in philosophy of science, that theoretical terms do not refer to anything in reality; the sentences in which they occur only serve as instruments for predictions. Thus the philosophical vogue and the Copenhagen interpretation by Bohr-Heisenberg supported each other.

The main argument for the collapse being a change of information state was the following: if we treat the measurement device quantum mechanically, thus describing its state by a wave function, we don't get any collapse. This is so because

[3] Two classical expositions of the Copenhagen Interpretation are Jammer (1974) and Jammer (1989)

the physical coupling of the measured object to the measuring device is described in accordance with the general rule for interaction between quantum systems, viz., by multiplying their respective wave functions. The resulting product, being a superposition of eigenfunctions to the applied operator, evolves in accordance with Schrödinger's time-dependent equation, i.e., continuously, deterministically and reversibly, but, alas, a well-defined value of the measured observable will never come about; the superposition will continue forever. But, certainly, when we observe the state of the measurement device we observe a precise value, viz., one of the eigenvalues. The conclusion that it is the human observer that performs the collapse just by looking, seems nearly certain.

But the human observer is also a physical object which can be described quantum mechanically, and the argument can be repeated just by coupling the observer's wave function to that for the combined object+measurement device; so the interaction between the observer qua physical object and the measurement device will not induce any collapse. The ultimate limit where this chain of interacting physical systems ends is the observer's mind. Hence, the seemingly unavoidable conclusion is that the collapse must be due to the non-physical aspect of the observation process, i.e., the change of the observer's mind state when acquiring new information. Von Neumann concluded that we must somewhere make a 'cut' between the chain of interacting quantum systems and the external world, classically described, but it is immaterial where it is done.

The measurement problem could thus be stated as follows: when we look upon and describe the measurement device as a quantum system, no collapse occurs, but when we look upon and describe it as a classical object, a collapse will occur. But, certainly, we observe the result of a collapse. All this suggests that the collapse is a change of information state on part of the observer.

But the tides have changed and classical instrumentalism is now rejected as an untenable position. Most philosophers of science nowadays adhere to some form of realism, the least common denominator of which is that truth transcends evidence: propositions are true or false independently of us knowing which is the case. (The stronger thesis that every observable at every point of time have a definite value is often held by realists, but it does not follow from realism as defined above, it is an independent assumption called *value determinism*.) It follows that statements about quantum states are true or false independently of which information observers have, hence the view that the collapse is a mere change of information state on the part of the observer conflicts with realism.

So the Copenhagen Interpretation is no longer popular and a number of alternative interpretations have been proposed. They can be divided into two main groups, collapse interpretations and no-collapse interpretations. The former accepts that the collapse of the state function reflects reality, i.e., that there really occurs in nature discontinuous, indeterministic and irreversible state changes, whereas no-collapse interpretations aim at showing that the collapse is a mere artefact of our theory and that all real state changes are continuous.

Schrödinger and Einstein were perhaps the first to express the latter view; Schrö-dinger is reported to have exclaimed at a meeting in Copenhagen 1929: 'Falls es bei dieser verdammten Quantenspringerei bleiben sollte, so bedaure ich, mich jemals mit der Quantentheorie beschäftigt zu haben!' (If these dammed quantum jumps are here to stay, I regret I ever start working with quantum mechanics!) Jammer (1989, p. 344). Many philosophers and physicists still have similar inclinations, which is the reason, I guess, that most interpretations that have been proposed in the post-Copenhagen era are no-collapse interpretations.

However, I'm inclined to the other view, i.e., to accept discontinuous state changes as real. Why are discontinuous state changes so abhorrent? And it is obvious that I'm not the only one thinking along these lines. Among those who accept that discontinuous changes are real are e.g. Ghirardi et al. (1986), Penrose (2005) and Sudbery (2002).

The more or less tacit assumption that all real changes are continuous has been prevalent throughout history; already in antiquity many denied the reality of discontinuous state changes ('Natura non facit saltus'). This belief got strong support from the very successful method of solving physical problems introduced by Newton. It may be described as: formulate a differential equation by giving the conditions for the state evolution of the system under scrutiny and then, by solving this equation, you get a set of functions, one of which can be selected (by determining boundary conditions) as describing the evolution of the actual system's state. The solutions are continuous functions and these are held to describe quantitative properties of physical systems. The method proved very powerful and was applied to virtually all dynamical problems in physics for more than 200 years. No wonder then, that most took for granted that all real state changes are continuous.

Hence, the assumption that nature must change continuously (and deterministically and reversibly), instead of discontinuously (and indeterministically and irreversibly) was supported by the success of science in using continuous functions as descriptions of physical quantities. But, as we all know, inductions sometimes fail. The question is whether we should rely on inductive evidence in this case; is the enormously successful use of continuous functions in classical physics and relativity theory reason enough to suppose that no real discontinuous state changes occur in the quantum realm? I think not. Prima facie, it seems to me just as plausible that state changes in the micro realm, i.e., in a regime at a much finer scale than in classical physics, are discontinuous, as they are continuous. The successful use of continuous functions in classical, macroscopic theories is just as easy to explain in both cases. Further, since the additional assumptions that are made in the many worlds interpretation, the pilot wave interpretation or in modal interpretations, which are the main alternatives among no collapse interpretations, are rather substantial (some would say they are much too far-fetched), one is prone to ask if it is not a better route just to accept collapses as real. Why not simply accept that natural changes may be discontinuous? The advantage is, as we will see, that no extra assumptions beside the basic postulates of quantum mechanics are needed, and we have no need for the projection postulate as an independent assumption. Moreover, as we will see in a moment, there are empirical evidence for discontinuous state changes, and this evidence is independent of any interpretation of quantum mechanics. But there are some difficulties to solve.

If we accept collapses as real state changes, we immediately confront the of question why they occur *only* during measurements, as von Neumann and his followers took for granted. However, empirical facts does not force us to believe *that*, only that a measurement (of the first kind) is a *sufficient* condition for collapse. I guess that von Neumann's reason to confine discontinuities to measurements was that he too believed that Nature does not change discontinuously.

The view that a measurement interaction is only a sufficient condition for a collapse has been proposed by Ghirardi et al. (1986). They postulated a new stochastic law saying that quantum systems undergo sudden state changes irrespective of measurements being made; the probability for such a change was left open but Ghirardi et. al. argued that empirical constraints left a reasonable interval for this probability. However, Albert (1992, pp. 92-111) has pointed out severe problems with this assumption.

I think Ghirardi et. al. was on the right track when rejecting the strict connection between collapses and measurements. Like Ghirardi et.al. I do think that collapses are not uniquely associated with measurements; rather they occur in many other situations as well, whether or not the object is measured upon. This is a consequence of the insight that there are no specific *physical* characteristics of measurements qua measurements. Measurements are only a subclass of physical interactions, a subclass characterised by the fact that the result is known by some humans. This conclusion becomes almost unavoidable when one remembers the fact that it is only measurements of the first kind, which cause any interpretative problem; measurements of the second kind can be given a completely classical account as passive observations of the state. (However, measurements of the second kind are exchanges of energy and maybe other quantities; I'll return to this issue in section 6.3.) Hence, it is not the fact that something is a measurement that is associated with the collapse, but that it is a measurement of *the first kind*. If the measurement is of the first kind, the prepared state is not an eigenfunction to the applied operator. It is this fact we should analyse more deeply when looking for an explanation.

4 Conditions on interpretations of quantum mechanics

Most people seem to require two things of a satisfactory solution to the measurement problem:

1. It should consist in a description of a physical mechanism of what happens during the measurement.

2. Sufficient and necessary conditions, formulated in purely physical terms (and not in terms of 'observation' or 'measurement') for the collapse, or for the appearance of one, should be given.

These requirements may seem entirely reasonable, since that is how satisfactory descriptions of state changes usually look like in physics. But what is meant by a mechanism? Presumably, it is a chain of events, or state of affairs, one followed by the next and each being caused by the preceding one in a orderly manner without no jumps. Hence this demand on an satisfactory explanation is ipso facto a demand that it be a continuous state evolution; so those who, like the present author, think we have independent evidence (i.e., evidence independent on any interpretation of quantum theory, to be explained in a moment) for there being discrete state changes in nature, and that the collapse of the state function during measurement is one species of such discrete changes, must dismiss this demand on an explanation. Instead, we should exhibit the evidence for there being discontinuous state changes, evidence that is independent on which stance one takes about measurements.

Secondly, if one thinks that we have no good reason to assume that all changes are deterministic, we should likewise dismiss the urge for necessary and sufficient criteria for the collapse. For if this process is a genuinely random event, there simply are no sufficient conditions, obtaining in advance of the collapse, the knowledge of which would enable us to predict the final state, nor, in fact, whether there will be a collapse.

Thus it is clear these two criteria rules out any collapse interpretation of quantum mechanic and leaves only no-collapse interpretations left for competition. But as already said, instead I dismiss the criteria.

One might hope that theoretical elaboration of the orthodox theory may illuminate measurement interactions and give us better understanding. But such elaboration is never a pure mathematical derivation of consequences of the postulates; assumptions about physical interactions from the environment, approximations, etc., are always involved, as in decoherence interpretations. This is certainly legitimate, but in effect it means developing the physics. This is not my purpose in the present paper; on the contrary, I take standard quantum theory for granted, and this is the reasonable stance for a philosopher of science. Quantum theory has now been with us for almost 90 years and there is not the slightest evidence for it being incomplete or wrong; we have very strong evidence for it being empirically adequate and, I would say, also for it being true, or very nearly so. (And the extension to relativistic quantum field theory covers much more phenomena.) The fate of the GRW interpretation (cf. (Albert, 1992) illustrates the risks of adding new empirical hypotheses. Moreover, being a realist in the weak sense that our purpose is to describe nature as being independent of human cognitive processes. Hence *my* basic criteria for an interpretation of quantum mechanics are:

Condition on a philosophical interpretation on quantum mechanics:

1. A solution to the measurement problem should not add any assumptions that changes the empirical consequences.

2. No reference to observers in the descriptions of the measurement process.

Thus the criteria for a satisfactory solution to the measurement problem differ between no collapse and collapse interpretations.

We may begin by considering the discontinuity of collapses: why do we need discontinuous state change descriptions in quantum theory? Which empirical findings has forced us to this move?

I submit that quantisation of interaction, discovered by Planck and reported in his famous (Planck, 1900), is the basic empirical fact which forces us to introduce discontinuity in the theory of the dynamics of micro systems. This means that the quantisation postulate replaces the projection postulate as a basic assumption in quantum mechanics.

5 Quantisation - The mathematical representation of discreteness of interactions

In textbooks in quantum mechanics the word 'quantisation' refers to the mathematical procedure of replacing continuous dynamical variables with Hermitian operators with (mostly) discrete spectra operating on Hilbert spaces. So for example, the classical momentum (in the one-dimensional case) $p = mv$ is in quantum mechanics replaced by the operator $i\hbar\partial/\partial x$. Mathematically, one difference between classical and quantum mechanics is that classical states are described using continuous real-valued functions, each having a definite value at any moment of time, whereas quantum mechanical states are given by (mostly) complex wave functions and these do not admit the determination of precise values of *all* observables; for some observables we can only give probability distributions over possible values.

These wave functions evolve continuously and no definite choice between the possible values is made. But upon observation we always obtain one of these possible values, which means that the state function in such a case undergoes a discontinuous change before or during the observation. This discontinuous change is represented by a projection operator. The reason why the replacement of dynamical variables by Hermitian operators is called 'quantisation' is that the set of possible values usually is discrete.

Quantisation is a mathematical procedure which is necessary for making empirically correct predictions about probability distributions in the quantum realm. Hence, there must be a physical feature of quantum phenomena that lies behind this need to replace continuous functions by operators with discrete spectra. This physical feature is the fact that interactions at the micro scale occur in discrete steps, discovered by Planck (1900). Instead of assuming that interactions between matter and radiation fields occur continuously, Planck assumed in effect that it was discretized and that enabled him to produce an empirically correct and unified theory for black body radiation.

Before Planck published his famous paper one had two radiation laws, Wien's and Rayleigh-Jeans'; Wien's law was empirically adequate for high frequencies, but not for lower ones, whereas Rayleigh-Jeans' law gave almost correct predictions for low frequencies, but not for higher frequencies. (It predicted that the energy density approached infinity as the radiation frequency increased, the so-called ultraviolet catastrophe.) Both laws were derived from classical assumptions, one of which being that energy exchange between matter and radiation fields were continuous. It was obvious that both these laws must be approximations of a more fundamental law valid for all frequencies. Planck found this law, now named after him.

In his derivation of this law he divided the energy spectrum into cells of size proportional to the frequency, and this is equivalent to assuming discrete state changes.[4] This was a new idea and the resulting law fitted all experimental results.

[4]Kuhn has claimed Kuhn (1978, 128-9) that Planck himself in fact never thought of his assumption as a substantial new physical hypothesis, viz., that interaction between matter and radiation fields occur only in discrete steps. According to Kuhn, when Planck attributed discrete energies 0, e, 2e, 3e....etc., to radiating oscillators in his Planck (1900) it was for him only a way of calculating the distribution. Planck assumed that the energies of individual oscillators were continuously distributed; he only assumed that the average energy of all oscillators in a particular interval was a well defined value proportional to the average frequency for all oscillators in that interval, i.e., $e = h\nu$. But the real significance of Planck's assumption is that it amounts to the assumption that oscillator energies are discretised. Planck himself does not seem to have understood that this step in fact was a new physical postulate; it was Einstein who in his Einstein (1905) clearly understood that Planck had introduced a revolutionary new physical principle.

An immediate consequence of discretised energy states of oscillators is that interaction between matter and radiation fields is likewise discretised. This is the fundamental fact which triggered physicists to develop quantum mechanics.

Another observed consequence of discreteness of interaction is the discrete lines in the emission spectrum from the elements, such as hydrogen, and this was in fact already observed before 1900. The hydrogen spectrum contains four such emission lines in the visible part of the spectrum, the frequencies of which satisfy the well-known Rydberg's formula. Rydberg published his formula 1888, but had no theoretical underpinning for it. It was Bohr who, much later, gave the explanation: the hydrogen atom can only change its energy in discrete steps. Since these well-defined energy steps are proportional to the frequencies of the emitted radiation during the change, the proportionality constant being Planck's constant, we have an explanation. And the same goes for other atoms.[5] So around 1912 it was established by the leading researchers that interaction between electromagnetic fields and matter is quantised, i.e., discrete. Here is how Bohr expressed this fundamental principle:

> …its [quantum theory] essence may be expressed in the so-called quantum postulate which attributes to any atomic process an essential discontinuity, or rather individuality, completely foreign to classical theories and symbolised by Planck's quantum of action. Bohr (1928, p. 581)

And Einstein wrote :

> It should be strongly emphasised that according to our conception the quantity of light emitted under conditions of low illumination (other conditions remaining constant) must be proportional to the strength of the incident light, since each incident energy quantum will cause an elementary process of the postulated kind, independently of the action of other incident energy quanta. In particular, there will be no lower limit for the intensity of incident light necessary to excite the fluorescent effect." Einstein (1905, 1965).

Thus we see that discreteness of interactions actually consists of two closely related, but distinct features:

[5]It is sometimes said that Bohr's postulate that electrons bound in atoms are in stationary states and only emit radiation during changes between such stationary states contradicts classical electromagnetism. In fact, it is no conflict unless one assumes that an electron in a stationary state is a particle orbiting the nucleus; for an orbiting charge must emit radiation according to electromagnetism. But saying that an electron is in stationary state in an atom doesn't entail that it is orbiting the nucleus; it may be conceived as a standing wave, in which case it does not orbit the nucleus and we have no conflict.

1. Every individual exchange process involves one object giving away energy and another object taking up energy (and possibly other conserved quantities) and this exchange is independent of every other interaction process. It is this aspect Bohr refers to when he writes that 'the quantum postulate attributes to any atomic process an essential individuality'. The individual exchanges between atoms and fields are all 'event atoms'. It means that an atom gives away one portion of energy (a photon) or takes up one such portion, no more and no less, in every exchange process.

2. The exchange process cannot be analysed further as a sequence of incremental changes. It is a discontinuous state change, either from the state (in quantum field notation) |ground state atom + excited field⟩ to |excited atom + de-excited field⟩ or vice versa. No intermediate states are possible; it is discontinuous.

Thus, Einstein's interpretation (cf. footnote 5) of Planck's radiation law and Bohr's interpretation of the discreteness of stationary states in matter could now be joined in the statement:

All interactions between matter and radiation fields occur in discrete steps.

But what about interactions between material systems without mediation of any field? Well, in reality no such things occur. We know that all interactions belong to one of the four fundamental forces of nature: gravitation, electromagnetism, weak or strong nuclear force, all being mediated by fields. For example, mechanical interactions between objects in contact, such as collision, friction, pull and push, are all electromagnetic interactions.

In classical mechanics and orthodox quantum theory interactions between two systems are described as direct exchange of momentum and other conserved quantities without mediation of fields.[6] But that is an approximation, albeit often a very good one. If an object gives away portions of energy to the electromagnetic field, and the same amount is taken up from the field by an object in the vicinity after a very short time, and no third object is so near that the probability for its interaction with the field needs to be taken into account, we can disregard the intermediate excited field state in the analysis. Moreover, the amount of exchanged energy in interactions between macroscopic bodies is far above that in a single quantum even in very fine-tuned experiments, which means that many quanta are exchanged during a short time interval. This means that a continuous function describes the process to a very good approximation and no effects of discreteness of interactions

[6]We have a theory, the standard model, which is a quantised theory for electromagnetism, the weak and the strong force. Furthermore, all involved seem to believe that also gravitational interaction is discretised, although no established theory exists yet. So there is intense research in the field of quantum gravity (string theory, loop quantum gravity) going on.

are observable.[7]

However, in micro physics the effect of quantization of exchange of energy becomes crucial; we cannot disregard the fact that the exchanged quantity is a field quantum, a portion of the conserved quantity, which in this interaction cannot be further divided. Hence, the conclusion is that the restriction to interactions between matter and fields could be omitted and we have a basic physical principle:

Principle of discreteness of interaction (PDI): *All interactions are discretised.*

A possible objection would be to say that systems sometimes interact without really exchanging portions of any conserved quantity. An electron passing through a thin metal plate, for example, can interact with the electrons in the metal by changing the Fermi levels around its path. It might exchange energy with the metal, but that is far from certain; it can also leave the plate with the same energy and momentum as when entering it, in which case the Fermi level returns to its former value. This passing through a metal without causing any permanent change might be called an interaction, but it is not an interaction resulting in an exchange of a portion of any conserved quantity, since the metal is in the same state after the electron's passage as before. In what follows I will use the word *'interaction'* for those events where the interacting systems exchange a conserved quantity, and thereby change their states, whereas cases where no such exchange occurs I will label *'influences'*. This difference is important to keep in mind in the discussion to follow.

Both Bohr and Einstein pointed out that discreteness of exchange of conserved quantities is represented in quantum theory by Planck's quantum of action. In other words, it enters theory with the well-known formula

$$\Delta E = h\nu \tag{12}$$

This formula tells us that any energy change ΔE in an atom, molecule or any piece of matter is proportional to the frequency ν of the radiation field interacting with that piece of matter and thus with the frequency of the emitted or absorbed photon. In other words, Planck's constant is the quotient $\Delta E/\nu$. This fundamental

[7]The first observed effect of quantization was, to my knowledge, the discrepancy between the calculated values for the fraction $\gamma = C_p/C_v$. for some molecules, for example HCl and observations. The reason is that the lowest excited vibrational energy level in these molecules is higher than available energy quanta, hence vibrational energy cannot be absorbed. Classical mechanics in contrast predicted storage of vibrational energy. The explanation of the discrepancies between predictions of classical statistical mechanics and observations was first available after Plank's introduction of the postulate of quantization.

feature of interactions makes it impossible to use ordinary continuous functions when describing the state evolution of a quantum system during such interactions; a continuous function cannot correctly represent discontinuous evolutions in time if the discontinuous steps are big enough. We need other mathematical tools. How do we find these?

6 From classical to quantum mechanics

Borrowing a concise formulation from Rovelli (2004, 14), to quantise is a technique for searching a solution to a well-defined inverse problem, viz., finding a quantum theory with a given classical limit.[8] So we use the correspondence principle as an adequacy condition on a quantum theory. It says that in situations when the total quantity of action is much bigger than Planck's constant, the predictions of quantum theory should be identical to those of classical mechanics.

Consider, as an example, a piece of matter made up of a large number of atoms, say 10^{23} or more. This object will have a very large number of densely packed energy levels. The result of this is that usually it can interact with radiation fields of almost any frequency. In other words, it would be hard, not to say impossible, to observe any difference between such a system and a classical one in which interactions were truly continuous. In other words, in the limit of big systems the new mathematics must give the same, or very nearly so, predictions as classical mechanics. We can formulate the principle as follows:

Correspondence principle: In situations where we can disregard the fact that Planck's constant is non-zero, i.e., where the discreteness of interactions can be neglected when calculating the values of variables, quantum and classical mechanics should make the same empirical predictions.[9]

How, then, do we find the correct quantum description of a particular state? The starting point in making the transition from classical to quantum mechanics is to replace continuous real-valued variables by Hermitian operators operating on classes of state functions describing quantum states. Hermitian operators are needed since they have real eigenvalues and the eigenvalues are taken to be the

[8] Although we regard quantum theory a fundamental theory and classical mechanics an approximation, we should keep in mind that from an epistemological point of view classical mechanics (and electromagnetism) is prior to quantum theory.

[9] There is also another version of the correspondence principle which says that in the limit of big quantum numbers the predictions of quantum mechanics should equal those of classical mechanics. These two versions are not strictly equivalent. For our purpose it is the first version which is useful.

observable values; surely, we observe only real values of observables.[10] Since the eigenvalues of some Hermitian operators in some situations are discrete, it is possible to construct a mathematical representation of discreteness of values of observables, with suitably chosen operators. These operators operate on state functions that represent states of physical objects.

One should keep in mind that we need not assume that *all* Hermitian operators have a counterpart among the set of observables of a system. It is only assumed that for any observable a Hermitian operator can be found. In this I follow what I take to be received view among physicists. For example, Dicke and Wittke (1960, 101) writes: "With every physical observable there is associated an operator."

Two observables that simultaneously do not have definite values are said to be *incompatible* and two such observables are replaced by non-commuting operators. If one such observable has a precise value before the measurement, those incompatible with it have unsharp values. Why is it so? Why can't we simultaneously determine all observables? The traditional argument stemming from Heisenberg (1930) was that all measurements by necessity induce an uncontrollable disturbance on the system. This view has been heavily criticised from the realist camp and I will not rehearse this criticism, but simply take for granted that those arguments are convincing. The alternative view, the *ontic* one, is that quantum systems simply do not *have* precise values on all observables at each point of time. This interpretation goes back to Margenau (1950, 1958) who reintroduced the Aristotelian distinction between potential and actual properties; Margenau's version was between *latent* and *actual properties*. The idea is that the values of unsharp observables are the latent properties until the measurement is completed. The measurement, if it is of the first kind, actualises a latent property. At the same time, actual properties become latent, a point often forgotten. (Potential/latent properties are such that they become manifest only under certain conditions; so they are dispositions.)[11]

Adopting the ontic view amounts to rejecting value determinism and conversely, rejecting value determinism is to adopt the ontic view. Quite a number of interpreters, perhaps the majority nowadays, seem to reject value determinism and thus implicitly adopt the ontic view. This is almost unavoidable once one dismiss the view that quantum states are observer's information states.[12]

[10]Thus I accept the validity of the eigenvector-eigenvalue link, in contrast to modal interpretations, in which it is given up.

[11]Jammer claims ([1974], p. 506) that Heisenberg's view in his [1958] was rather similar to that of Margenau on this point; Heisenberg wrote that 'a transition from the possible to the actual takes place during the act of observation' (ibid. p. 54), But this is quite different from Margenau's view, for Heisenberg talks about information states of observers, not about properties of quantum systems per se.

[12]Think of a simple double slit interference experiment with electrons; we know that behind the double slit we will obtain an interference pattern incompatible with the assumption that every elec-

Every preparation of a quantum system, properly so called, results in a division of its observables into sharp and unsharp ones, corresponding to actual and potential properties. This is so because the preparation consists in selecting and bringing under experimental control systems with well defined states, i.e., systems described by a known wave function and this function is an eigenfunction to some operators, while not an eigenfunction to others.

7 Operators, interactions and observables

7.1 Hermitian operators and projection operators

The common view is that some Hermitian operators operating on state vectors in Hilbert spaces *correspond* to, or *represent* observables. These expressions are not entirely appropriate, if we take the ontic view on unsharp observables, thus calling them rather 'indeterminate variables', because the words 'correspond' and 'represent' suggest that the system in question *has* values of observables, albeit unknown, which is not the case according to the ontic view; indeterminate observables get determinate values as a result of irreversible state changes.

But a Hermitian operator does not bring about that kind of change. We use Hermitian operators to calculate probability distributions for observables, i.e., with their help we can determine the probabilities for different possible outcomes of measurements, but not for determining which state an individual quantum system will enter into upon measurement. For example, a spin-1/2-system being in the state 'spin up' in the y-direction will still be in an eigenstate to the spin-y-operator, hence not in an eigenstate to the spin-z-operator, when this operator has acted upon the state vector. This is easily recognised from the following equation, where the 2x2 matrix is Pauli's spin-z-operator that operates on the state vector representing spin-up in the y-direction (spin-up and spin-down in the z-direction are chosen as base states and Planck's constant is omitted):

$$\begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix} \frac{1}{\sqrt{2}} \begin{pmatrix} i \\ -1 \end{pmatrix} = \frac{1}{\sqrt{2}} \begin{pmatrix} i \\ 1 \end{pmatrix} = \frac{i}{\sqrt{2}} \begin{pmatrix} 1 \\ 0 \end{pmatrix} + \frac{1}{\sqrt{2}} \begin{pmatrix} 0 \\ 1 \end{pmatrix}$$

Thus, the spin-z-operator does not reduce the superposition to any of its eigenstates, it merely changes the relative phase between the components. The end state is still an eigenfunction to the spin-y-operator, and it can be transformed back to the original state by another operation of the spin-z-operator. To represent the change

tron passes one of the slits. Hence, the electrons cannot be attributed any definite position at the passage, hence the observable 'position' has no definite value at all moments in this experiment.

from a superposition to a definite value in the spin-z-direction we need a projection operator.

The formalism of first applying a Hermitian operator and then a projection operator to a wave function represents a complete measurement on a system being in a state described by that wave function. Now, I suggest that these two steps corresponds to two stages of the measurement process.

A measurement may be described as a three step process. In the first step an ensemble of objects, all of which being in the same well defined state, is produced. Second, the objects in this ensemble are directed into some apparatus, e.g. a magnet if a spin measurement is to be performed, that has some *influence* (i.e., it does not exchange any conserved quantity with the measured object) on these objects, an influence which affects the spatial distribution of the function describing the state. The function of this step is to establish a correlation between the observable to be measured and a directly observable variable, usually positions of possible detections. Finally, in the third step, these objects, or some secondary objects that are hit upon, or produced by the original objects, are detected at these correlated positions.

Several authors,, e.g., Margenau (1958) and Cartwright (1980) claim that all measurements ultimately are position measurements, and if they are right, which I think they are, no matter which observable one wants to measure, one needs to establish a correlation between the observable of interest and positions of detections. So the second step is necessary.

If we now compare this description of a measurement with the formalism of quantum mechanics, it seems very reasonable to view the second step as represented by a Hermitian operator and the last step by a projection operator. So these two kinds of operators could be viewed as representing something *done* by the environment on the object measured upon, irrespective of whether a human has constructed the environment or not.[13]

In the spin measurement discussed above, where spin-half particles having spin-up in the y-direction are directed through a inhomogeneous magnetic field, i.e, a SG-magnet, oriented in the z-direction, it is the influence of the magnetic field on the system that may be thought of as corresponding to the action of the spin operator on the state function.

[13]This view on operators is somewhat analogous to do-operators, so-called by Judea Pearl in his (Pearl, 2000). These operators he introduced as a means to identify causal relations among conditional probabilities. A do-operator, as defined by Pearl, has the form do($X = x_i$) with the meaning that the experimenter by experimental means has fixed the variable X so that it always has the value x_i. So a do-operator always represent a human interaction. In this respect they differ from how I view Hermitian operators and projection operators; these need not represent human actions. But the similarity is that they represent *actions* on systems.

The inhomogeneous magnetic field separates the beam into two (in reality the separation is never total!), and the particles can then be detected at two reasonably well separated places; if detected in the upper beam they are attributed 'spin up' , whereas those in the lower beam are attributed 'spin down' relative to the direction of the magnetic field. Hence the point of using the magnetic field is to establish a correlation between spin values and positions of detection.

One might now be inclined to think that it is the action of the magnetic field that gives the particles a definite 'spin up' or 'spin down' state, so that this action would be an *interaction*, in my use of this word, i.e., an exchange of momentum, between the magnetic field and the particles. But that is not so, if our assumption that the spin-z operator represents the action of the magnetic field, because the superposition still exists. In the formalism the particle gets a definite spin in the z-direction later, when an action represented by the projection operator, i.e., the detection, takes place.

It is generally believed (see for example Wigner (1963), Omnés (1994) and Albert (1992)) that the effect of the magnetic field can be undone by another magnet. The argument is that in the formalism one can apply once more a spin-z operator and restore the original state. So if we reunite the two beams behind the magnet without affecting the spin state, which can be done since the operators for spin and linear momentum commute, and then measure spin, we will with probability equal to one get definite spin in the originally prepared direction, i.e., in the y-direction, not in the z-direction. In other words, the mere passing through a magnetic field does not induce the collapse. So the action of the magnetic field on the system is an *influence* in my use of this word; the environment changes the spatial intensity distribution of the wave function, but it does not perform any collapse.

Wigner (1963) has discussed this and he comments (op.cit., p. 10): 'Even though the experiment indicated would be difficult to perform, there is little doubt that the behavior of particles and of their spins conforms to the equations of motion of quantum mechanics under conditions considered.' Omnés agrees and writes: 'This result unfortunately has to be accepted without any experimental check because the experiment is too difficult to perform.' (op.cit., p. 72).

If we were to align the magnet in another direction we should apply another operator. So there are fairly good reasons to say that the spin operator represents the influence of the magnetic field on the system represented by the given wave function, but not the interaction where the system irreversibly changes to the final state. The difference between these two kinds of actions is that the first one is reversible and the second not.

I believe we can generalise and say that all those Hermitian operators found by using the correspondence principle represent, when operating on wave functions, reversible influences from the environment onto systems represented by these wave functions. The influence on the system represented by the Hermitian operator is thus a change of the latent properties, i.e., the probabilities for different possible state changes. The collapse of the wave function during the final detection is represented by a projection operator, which erases all but one component in the superposition and is independent of the action of the Hermitian operator.

There is no reason to assume that all operators may be interpreted in the same way. Operators are mathematical objects that map functions onto other functions. Some such mappings may be viewed as representing something done with the state functions some others not; as argued in section 2, parity and time reversal operators represent changes of coordinatisation. It is only *some* Hermitian operators, viz., those we use for calculating values of observable quantities, and projection operators, that may be looked upon as representing actions on physical systems, i.e. influences and interactions respectively.

According to the Copenhagen interpretation, the collapse occurs *only* during measurements. The analysis given here does not support that view; there is no reason to suppose that collapses only may occur if someone observes the outcome of the collapse. As already said, there are no good arguments for thinking that measurements, i.e. physical interactions where a conscious observer becomes aware of the outcome, should have any particular physical characteristics qua measurements. Hence it would be correct to apply a projection operator also when there is an unobserved state change. But since such state changes are indeterministic, we cannot know which projection operator to use unless we observe the outcome. (In physics this is no issue since one almost never is interested in the state of only one individual system; one studies distributions of states in ensembles of systems, because hypotheses are tested by comparing observed and predicted probability distributions, and since the Hermitian operators give probability distributions of observable values in ensembles, this is all fine. The individual events are rarely of any *practical* interest for the scientist; but it poses a conceptual problem, a problem of interpretation.)

7.2 The traditional account of interactions

The usual way of representing interactions between a quantum system and a measurement device, (or any other object for that matter) for example in von Neumann (1996) and Wigner (1963), is to multiply their state functions.[14] This is defended

[14] In this paper Wigner treats microscopic objects as 'apparatuses'; in the spin measurement example he calls the position coordinate of a spin particle the 'apparatus' which measures the spin, in another example he treats a particle colliding with the measured object as a measurement device. This seems to me illegitimate, since these things cannot enter into pointer states stable enough to be repeatedly read off.

by saying that there is a one-one-correlation between the states of a measured object and those of the measurement device; hence when observing the state of the measurement device one can infer the state of the measured object. But how is this correlation established in practice?

In the case of a spin measurement, the state of the measurement device, as represented by a state function, must be the state of the two detectors taken as one single object, for the state *spin-up* is correlated with a detection in the upper detector and the state *spin-down* with a detection in the other detector. Each of these two macroscopic devices can be in two states, triggered or not triggered, and so the outcome space of the experiment ought to consist of four states: (triggered, triggered), (triggered, not triggered), (not triggered, triggered) and (not triggered, not triggered), where the first component in each outcome refers to, say, the upper detector and the second component to the lower one. If so, no correlation between spin state and the state of this device would obtain; hence we need to somehow remove the two possibilities (triggered, triggered) and (not triggered, not triggered) from the outcome space to get a one-one-correlation between spin states and detector states.

The outcome (not triggered, not triggered) would have zero probability if both detectors had 100% efficiency. But as far as I know, detectors are not thus perfect. Perhaps one could say that in practice this case is simply not counted as a possible outcome; for predictive purposes these events are not part of the play. If a particle is sent through the device and nothing happens, this run is simply not counted. But from a conceptual point of view one could not dismiss these cases so easily.

Next, how is the possibility of both detectors being triggered ruled out? If such a case were observed, I guess that defenders of the classical measurement theory would say that this was due to the fact that two particles had hit the detectors so close in time that we can't separate their arrival time, relying on the fact that emission of particles from the preparation part of the equipment cannot be perfectly controlled. But also this defense seems to me not very satisfying from a conceptual point of view, for it leaves us with no defense for the classical view that the measurement device when connected to the spin particle enters into a correlated state of superposition together with the spin particle.

The two arguments above only shows that it is our way of counting events, which makes the outcome consistent with theory. The correspondence in constructed by a careful use of rules for counting events in the statistics. Hence the correspondence is an artefact.

A defender of the traditional view might perhaps answer that when the spin particle enter the detecting device the latter somehow adjusts its quantum state to the incoming particle. But that begs the question for we have no independent reason for this assumption. Moreover, how could two macroscopic objects (the two detectors) suddenly be put into a coherent quantum state? It may be possible, but we certainly need an account of how two macroscopic devices at room temperature and at some distance may enter into a coherent quantum state.

I guess that the traditional view is based on the tacit assumptions that i) the spin particle goes either up or down after passing the SG-magnet, ii) is in the vicinity of one detector just before detection, and iii) that a particle only can trigger a detector in its immediate vicinity. But these assumptions entail that the collapse occurred when passing the magnet and that the particle, just before detection, either is approaching the upper or the lower detector. But that must be wrong; for it would mean that it is correct to attribute a well defined position in the z-direction to the particle even before detection; and that runs into conflict with the well-known fact that we cannot equate the probability *for detection* at at particular place and the probability *to be* at that place; for if we could, it would be possible to attribute well-defined trajectories to particles even when passing unobserved through for example a double-slit, which is false, see e.g. Cartwright (1980). Quantum particles do not behave as particles having well defined positions at each point of time during propagation in space; they propagate in a wave-like manner.

A further point about the distinction between system and environment (we may call the measurement device a part of the environment for the system to be measured) may also be made. When we describe the dynamics of a particular system we represent the impact of the environment upon this system by potentials being part of the Hamiltonian and the impact from these potentials is treated classically, i.e., as a continuous influence on the dynamics of the quantum system. So orthodox quantum mechanics is actually a semi-classical theory since not all interactions are quantized. Hence when using orthodox quantum theory we always make a choice, a cut between the system under scrutiny and the rest of the world. This is of course true also in classical physics, so why does it cause conceptual difficulties in quantum theory only? The reason is that measurement interactions belong to the class of events accounted precisely for by quantum theory.

It is instructive to compare measurements inducing a collapse with so called protective measurements (Aharonov and Vaidman, 1993, 38-42) . According to Aharonov &Vaidman a protective measurement is a measurement where the interaction is so weak that the interaction Hamiltonian approaches zero as times goes to infinity. This means that the energy change of the measured object is so small that the energy before and after the measurement is nearly the same, which means that the effect of quantization can be neglected. Hence such a measurement, even when treated quantum mechanically, appears to be almost classical in character (and the result is the expectation value, not one of the eigenvalues).

Now suppose we change perspective and include a part of the environment in the system. Then we need no projection operator for representing an exchange of a conserved quantity, since the exchange occurs between parts of this enlarged system. So long as we don't observe one part of the combined system only, no collapse is observed. So this account suggests that the collapse is a mere change of information on the part of an observer.

But the evidence don't support that conclusion. We observe a collapse when a conserved quantity has been exchanged between the system under scrutiny and some part of the environment. If we include that part in the system under scrutiny, we don't observe any collapse; but that doesn't entail that no collapse has occurred, of course; it has still occurred, but our way of individuating things miss it. This is no special feature of quantum theory; in order to observe the internal dynamics in a system we must observe component parts of the system and be able to attribute properties to these individual parts independently of the properties of other parts of the system.

As already remarked, von Neumann argued that one could consistently include the wave function for the observer in the quantum mechanical description of the process and concluded that we have to make a cut between what is described by a quantum mechanical wave function and what is outside, but that it is immaterial where to draw the line. I don't think it's immaterial. The concept *measurement device* is a functional concept, not a physical concept. It means that we identify whatever object that fulfills the appropriate function as a measurement device. Among the conditions on such things is that their pointer states should be stable under repeated observations under normal circumstances in order to make intersubjective agreement of observations possible. So the measurement device should not be given a quantum mechanical description. (This conclusion is analogous to Bohr's stance that reports of observations must be made in classical terms.)

But, a defender of von Neumann's position could say that the measurement device is a physical object, and that it *could* be given a quantum mechanical description. Agreed. He could further argue, using the decoherence programme, that there is an account of its continuous development into stable macroscopic pointer states due to dissipation of information into the vast number of degrees of freedom. Yes, that's possible. But, as repeatedly pointed out, we have evidence for discreteness of interactions, independent of any interpretation of quantum theory, and this evidence, for example discrete emission lines in the spectra from the elements, don't depend on any account of macroscopic devices. The defender could now reply that the evidence surely is got from observations using measurement devices, and hence, the discreteness of emission lines is a fact first when detected in a measurement device. I don't accept that conclusion, it is too much instrumentalism for me.

The conclusion to draw is that, because of quantisation, the choice of where to draw the line between system under scrutiny and environment has implications for the description of the dynamics; if we study a system which does not exchange any conserved quantity with the rest of the world, or where the exchange is negligible in comparison with the measured value, we may describe its evolution by continuous functions, but if it does exchange a conserved quantity of more than negligible magnitude, we need discontinuous functions.

Sometimes we usefully apply continuous functions (solutions to the Schrödinger equation) even though the system under scrutiny exchanges energy and/or momentum with the environment. In a piece of metal, for example, the energy levels can be so close that one can represent its energy spectrum as continuous, which means that even extremely soft photons may be absorbed. Then we can safely apply the correspondence principle and treat the interaction classically.

The same can sometimes be said about a single particle's interactions. Such a particle may absorb, or emit, soft photons, thus increasing or decreasing its kinetic energy with a very small amount, small enough to justify neglecting the change. Suppose for example that energy change is 1/10000 of the actual state. For most purposes we could disregard the change of the energy state, and hence not representing the evolution as a collapse. This situation can be iterated many times; the particle may absorb, or emit, during a certain time span, a huge number of very soft photons. Since quantization prohibits the absorption or emission of many photons at the same time (see the Bohr quotation above), the state changes are fundamentally a sequence of discrete jumps, but for all practical purposes we may describe the evolution by a continuous function, because the steps are very small. Measurements of this kind of events were called 'protective measurements' by Aharonov and Vaidman (1993), as referred to above.

The basic postulate of the standard model is that electromagnetic, weak and strong interactions are discretized. This means that all interactions of these three types occur in discrete steps. If we believe in the standard model we must say that continuous evolution during interactions with the environment is a useful approximation, but that, fundamentally, the evolution during interaction is discontinuous.

7.3 Unobserved interactions and influences

I guess that no one will contest the assumption that Nature just by itself, without any human intervention, is such that a quantum system may enter into a physical environment that physically resembles those which we may arrange in experiments. Not that Nature itself would isolate one portion and call it a 'quantum system', of course, but the physical interactions we try to describe would be the same. Take for example, again, the Stern-Gerlach experiment described above. Certainly, nature is full of i) inhomogeneous magnetic fields, ii) electrons passing such fields, and iii) macroscopic objects that take up energy from incoming electrons. Having accepted this, we have in fact also accepted that spin and projection operators in principle could be used for describing such events, be they measurements or not.

In classical physics we need not use operators when representing actions on physical systems. This fact suggests that in this domain it is possible to determine values of physical quantities by passive observations without doing anything with the observed systems. But this is wrong; any observation is an exchange of photons (or maybe other field quanta), so there are, strictly speaking, no such thing as an entirely passive observation. The reason why we nevertheless can describe measurements in the classical domain without using operators representing interactions is that the quantities exchanged in those observations are negligible in comparison with the observed values; a huge bunch of quanta is exchanged and the process may, for all practical purposes, be thought of as a continuous and gradual process in time; the values of the observable before and after measurement are, within the limits of measurement errors, the same. Hence we need not bother about the quantisation of interaction. Classical mechanics and classical electromagnetism are for very many purposes good enough theories.[15]

What then about a measurement of the second kind, i.e., a measurement which is not associated with a collapse? Surely it must consist in exchange of quanta of energy, i.e., photons, between the detector and the measured object. So it is not a completely passive observation after all. The reason why we don't represent this interaction by eq. (1) is that the prepared state is an eigenstate to the Hermitian operator representing the type of influence done on that system. For example in spin measurement of the second kind, no influencing magnet is introduced between the preparation and detection, so the detection is merely a confirmation that the preparation has succeeded. But a detection is made and in that process a quantum is exchanged. So, exchange of quanta of energy is not always represented in our descriptions of what happens;, but collapses, i.e. state changes represented by eq.(1) are always connected to exchange of quanta of energy.

7.4 Measurements of continuous observables

An anonymous referee asked whether the generality of discreteness of interactions is compatible with the fact that there are continuous observables.

The answer is yes, but first of all I think there is reason to doubt a seemingly tacit assumption behind this question, viz., that physical objects in reality have

[15]It may be observed that from an epistemological view we use the correspondence principle to find operators and construct quantum theory. But from an ontological view we reverse its use. The behaviour of macroscopic objects is explained starting from quantum theory. When we go to the macroscopic domain, we make the approximation that Planck's constant is zero, which will give us classical mechanics.

continuous observable properties; the reason for doubt is based on the assumption, shared by virtually all physicists it seems, that a fundamental theory of gravity must be a quantized theory; hence, space and time, and and all functions of space and time parameters, are fundamentally discontinuous. If so, continuous functions are useful approximations.

But let's forget about that and assume that e.g. position along the x-axis is a continuous observable. The corresponding operator is **x** which has a continuous and infinite set of eigenvalues. Doesn't this conflict with the statement that interactions are discretized? No.

In order to measure the position of an object we must exchange a conserved quantity, such as energy and/or momentum with it. The place of exchange, i.e. the position of the detector, is correlated with the value of the observable in a given frame of reference. Now, obviously, we cannot set up a row of independent detectors infinitely close to each other, so there are practical restrictions. But let's also forget about these, assuming the possibility of having an infinite number of detectors between any randomly chosen two ones, so that any possible position can be obtained. Still a detection is required and the detection is an exchange of a conserved quantity between the object whose position is measured and the triggered detector. So there is no conceptual conflict between assuming continuous observables and discreteness of interactions.

Hermitian operators replacing observables (using eq. (3)) may, when acting on state functions, arguably be viewed as representing types of influences, irrespective of these influences being part of measurements or not. Hermitian operators *replace* continuous variables, when we go from classical to quantum mechanics, they do not *represent* them. We need to know what type of influence is needed in order to measure the value of a particular variable. But then, how to find operators that, when applied to state functions, represent such influences?

7.5 The formal counterpart to the Correspondence Principle

Quantum theory does not provide us with any algorithm for finding the correct Hermitian operator to replace a given dynamical variable. But the correspondence principle guides us. It can be given a mathematical expression by requiring that the commutator for two operators **A** and **B** be proportional to the Poisson bracket for the corresponding classical variables A and B, with $i\hbar$ as the proportionality constant:

$$[\mathbf{A}, \mathbf{B}] = i\hbar\{A, B\} \tag{13}$$

(The fact that the imaginary i pops up is interesting in itself and indicates that we now need to use complex functions as state functions, but I will leave this aspect aside here.)

In their classical textbook Dicke and Wittke (1960) states eq. 3 as a postulate of quantum mechanics. But after introducing it as their last postulate (postulate 7, p.102-3), they continue:

> "Two observations should be made in connection with this postulate. The coordinates and momenta must be expressed in Cartesian coordinates. Also, in certain cases, ambiguities can arise in the order of non-commuting factors. These can often be resolved by remembering that the operator must be Hermitian. Because of these limitations and ambiguities, this 'postulate' must be regarded more as a helpful guide than as a basic postulate of quantum mechanics.... Postulate 7 [i.e., Eq. 3] may seem strange. However, note by direct substitution that postulate 7 is correct for the six components of r and P taken in any combination, and for any positive integral power of *r* and a component of *P*, and vice versa. In chapter 8, when the time rate of change of the expectation values is considered, it will be found that this postulate represents an important bridge between classical and quantum mechanics."

Thus, Dicke &Wittke is quite clear about the status of eq (3) as a correspondence rule, a necessary condition on any suitable quantum theory.[16]

8 A thought experiment

Let us assume that we have an atom in a radiation field with given frequency. Let us further treat h as a variable. This is possible since the value of Planck's constant is not determined by any postulate of quantum mechanics; it is an empirically determined parameter. Since $h = \Delta E/\nu$, if $h \rightarrow 0$, then $\Delta E \rightarrow 0$, with given frequency. (The arrow should here be taken to mean 'approaches', not as taking the continuous limit as in calculus.) This means that if h were equal to zero, there would be no energy spacing at all in the interactions. This would mean that the atom, or whatever system we consider, could absorb or emit any amount of energy. That is to say, if the frequency in the radiation field is given and non-zero, h determines the possible energy changes upon absorption or emission from a molecule, atom, or any piece of matter. Thus, the value of h contributes to determining the energy steps in a material system.

[16]One could alternatively start with the two postulates $\mathbf{x}\psi(x) = x\psi(x)$ and $\mathbf{p}\psi(x) = -i\hbar\frac{d\psi}{dx}x$, from which other operators could be guessed. But one should remember that it is in general *not* true that if A=f(B), then **A**=f(**B**), see Redhead (1987, ch. 5)

But, the reader might ask, isn't it the case that quantisation primarily is due to the fact that only certain energy states in atoms and molecules can be stable or 'stationary', using a term introduced by Bohr. If so, Planck's constant doesn't determine anything.

Of course, the energy spacing in a material system depends on the value of Planck's constant. But there is no order of priority, neither logical nor causal, between the fact that energy spacing in matter depends on the value of Planck's constant and the fact that state changes are quantised; since energy spacing in systems of matter is discretised, it will absorb or emit definite quanta, and if it only can absorb and emit definite quanta, its energy spacing must be discretised. In other words we have

> **Conclusion 1.** $\Delta E = h\nu$ is a mathematical expression for the fact that all interactions by which conserved quantities are exchanged between radiation fields and matter systems are discrete.
>
> **Conclusion 2**. Discreteness of interaction entails that $h > 0$.

Now it is time to rehearse three well-known features of quantum mechanics:

> **Lemma 1**: If $h \neq 0$ then not all Hermitian operators replacing classical variables commute.[17]
>
> **Lemma 2.** If two operators don't commute, there exists no complete set of functions spanning a Hilbert space on which the operators operate, such that all functions in the set are eigenfunctions to both operators.[18]

Thus it follows that there exists non-commuting Hermitian operators defined on the relevant Hilbert space, which means that there are state functions in this

[17] Proof by contraposition. Assume Eq. 12. Assume also that all Hermitian operators replacing classical variables commute. Then since the Poisson bracket for some pairs of classical variables (now identified as observables) differs from zero, (for example x and p_x) we have that for such a pair (A, B) with corresponding operators **A** and **B**, $\{A, B\} \neq 0$ and $(\mathbf{A}, \mathbf{B}) = 0$. Since $(\mathbf{A}, \mathbf{B}) = i\hbar\{A, B\}$ (eq. 12), it follows that $\hbar = h = 0$. Hence, assuming Eq. 3 as a necessary condition on any acceptable quantum theory, and $h \neq 0$, it follows that not all Hermitian operators replacing classical variables commute.

[18] Proof by contraposition. Suppose i) Ψ is any function belonging to a complete set spanning a Hilbert space $\mathfrak{H}$ and ii) that Ψ is an eigenfunction to two operators **A** and **B** with eigenvalues a and b respectively, i.e., $\mathbf{A}\Psi = a\Psi$ and $\mathbf{B}\Psi = b\Psi$. Then $(\mathbf{A}, \mathbf{B}) = \mathbf{AB}\Psi - \mathbf{BA}\Psi = \mathbf{A}b\Psi - \mathbf{B}a\Psi = b\mathbf{A}\Psi - a\mathbf{B}\Psi = ba\Psi - ab\Psi = 0$. Since this is valid for all functions in the set spanning the Hilbert space, we have that **A** and **B** commute. Hence if they don't commute, there exists no complete set of eigenfunctions to both jointly.

Hilbert space that are eigenfunctions to one set of Hermitian operators but not to another set. By this we have established that that the antecedent in the following conditional must be fulfilled for some state functions.

> **Lemma 3.** If a system S is in state Ψ being an eigenstate to an Hermitian operator **A**, but not to another Hermitian operator **B**, then S will make an discontinuous state change, if it evolves into an eigenstate to **B**.

This is a well-known feature of quantum theory and the proof was given in section 1. We have also earlier established that discontinuity, randomness and irreversibility is a package deal in quantum theory; hence if a state change is discontinuous, it is also random and irreversible, it is a collapse.

The evolution into an eigenstate to **B** is represented in the formalism by a projection operator. This evolution need not occur, even if the system has been influenced by the environment in a way represented by operator **B**. This is so because the influence from the environment need not result in any exchange of energy or some other conserved quantity. For example, a photon hitting a metal surface may be reflected, pass through or being absorbed and it is not possible to arrange things so that only one of these events will occur with 100% probability. If it passes through, neither the photon nor the metal plate change state, so this event will not be any collapse. Hence, whether the collapse occurs or not cannot be predicted with certainty, which means that we cannot tell in advance whether we, in a particular case, should apply a projection operator; it can only be done ex post facto.

Measurements have the specific epistemic property that the result is known, in contrast to all other interactions. This makes a difference when calculating probabilities. First, we simply don't take into account those cases where the system to be measured fails to trigger any detector, hence the probabilities we measure are of the form 'the probability for the system to enter into state ϕ_i, conditional on it interacting with the detector.' (Remember that 'interact' here means 'exchanges a portion of a conserved quantity'.) But this of course does not entail that it *must* interact with the detector, even though the sum of all probabilities is 100%. Second, we can only apply a specific projection operator when we know the outcome. So we have good reasons to believe:

> **Conclusion 3.** If two Hermitian operators **A** and **B** don't commute, i.e., $[\mathbf{A}, \mathbf{B}] \neq 0$, and if a system Ψ is in an eigenstate to **A**, it may collapse when an influence on Ψ represented by $\mathbf{B}\Psi$ is followed by an exchange of a conserved quantity between the environment and the system, independently of this exchange being a measurement or not.

Summarising, we have a good argument that any theory constructed so as to correctly represent discrete state changes must include collapses:

> **Conclusion 4.** Discreteness of interaction entails that interactions involving exchange of a quantum of a conserved quantity between a quantum system S and the environment will result in a collapse of the state function for S, whether or not this interaction is a measurement.

9 Mechanism for collapse?

I take this account of quantum interactions, and in particular measurements, to be explanatory. Many would disagree, I guess, asking for a mechanism that tells us when the collapse will occur, and which of the possible final states will be realized. But this demand is incompatible with accepting discreteness of interaction as a basic postulate in quantum theory. For if changes of dynamical states really are discontinuous and we represent such states as functions in Hilbert space these state changes are indeterministic, as shown above, and no matter how detailed description of the state evolution we are able to come up with, discontinuity and indeterminism cannot be avoided.

A state function representing the state before a collapse contain no information telling us whether the system actually will collapse in a certain environment or not. We might arrange a situation so that collapse is almost certain; in the case of a measurement that would mean we have construed a detector with near 100% efficiency. I don't know if this has been done, or is possible; but the crucial thing is that the state of the quantum system don't determine the probability for collapse; the collapse may occur in a certain environment as a result of interaction with that environment while not occurring in other environments. Hence it is not possible to state necessary and sufficient criteria for collapse without specifying properties of the environment. It may be the case that nothing happens and the system continues on its path without interacting with the environment and hence without collapsing. If that happens, we will never care about it, referring to the fact that detectors have less than 100% efficiency. And of course, in the natural course of events where no humans are interested in observing anything we have no information about it.

The passing through a detector without affecting it is an instance of tunneling, and it is possible to calculate the tunneling probability. It is zero only if the tunnel potential is infinite, which it cannot be in any real case, albeit one can construe situations making the probability as close to zero as one wants. Assuming that the state function is a complete description of the state, the probability for tunneling reflects genuine indeterminacy, not incomplete information. If so, one cannot give necessary and sufficient conditions for tunneling, and hence nor for collapses, to occur.

Indeterminism means that a complete state description does not contain any information about which of the possible final states will be realized. Since we represent the collapse in the mathematical formalism by a projection operator, it means that we cannot determine in advance which projection operator to apply in a particular case; it is obvious that if we could, the evolution would be deterministic.

Moreover, as we saw, indeterminism follows from the way discreteness of interaction is represented in quantum theory. Hence, one cannot consistently accept collapses as real and observer independent events in the world and at the same time ask for a continuous mechanism or for a description of the exact conditions for collapse. Either we have to deny that collapses are real events and ask for a continuous, account of interactions, or accept that collapses are real and accept irreversibility, indeterminism and discontinuity. I opt for the second alternative, since we have independent reasons to believe in quantisation of interaction.

When a system has collapsed into one of the eigenstates of a Hermitian operator representing the influence from then environment, it may continue existing as independent system. (An electron may, for example emit a photon in a detector and continue its path out of the detector.) If so, this new state may of course also be described as a superposition in terms of base states that are eigenstates to operators that do not commute with that representing the previous influence. Hence the collapse may repeat later in another environment. The time evolution of such a system may be described as piecewise continuous, i.e. continuous during time intervals separated by discontinuities.

We need not the projection postulate (remember that it says that quantum systems collapses when being *measured*!) as an independent assumption; it is replaced by the postulate of discreteness of interaction, and this well confirmed empirical fact has since the very beginning of quantum theory been the very basis of this theory in the sense that it was this fact that forced inventing quantum theory in the first place.

10 New predictions?

No one has, as far as I know, claimed that standard quantum theory (with its extension to QFT) is empirically wrong, that its predictions deviate from observations on some point. On the contrary, nearly 90 years research has not given the slightest evidence that it is incorrect. So I think a reasonable point of departure for discussions about interpretation should be not to add new physical assumptions, because that would enable predictions differing from standard quantum mechanics. I do think we now have sufficient evidence for saying that quantum theory in its present state is true, or very nearly so. Being a realist I conclude that it must be possible to interpret this theory in a way that doesn't involve human observers in the dynamics and without adding any assumptions entailing any empirical predictions which could be made without such assumptions. This is my point of departure; we should try to better understand quantum theory without changing its empirical content.

Hence, no new physical assumptions are made in this interpretation. In fact, the core idea in this paper is that von Neumann's postulate is superfluous; collapses are consequence of discreteness of interaction and discreteness of interaction is a basic postulate in quantum mechanics. That interactions occur in discrete steps are perhaps hard to understand; but if we accept that such changes occur, which we must after Planck's discovery, we must accept the consequences, viz., that such interactions are discontinuous, indeterministic and irreversible. Hence collapses are physical facts. The only special thing with measurements is that we humans observe the outcome.

11 Summary

The argument in this paper can be summarised as follows.
1. It is a well established empirical fact that all exchange of energy and other conserved quantities between two systems is quantised.
2. This fact is in quantum theory represented by the equation $\Delta E = h\nu$.
3. Therefore state functions describing states of systems change discontinuously when exchanging energy and other conserved quantities with the environment; and the discontinuous change can be approximated by a continuous evolution only if one can disregard the fact that Planck's constant is greater than zero.
4. In quantum theory, if a real state change (not a mere change of reference system) is discontinuous, it is also indeterministic and irreversible.
5. A discontinuous, indeterministic and irreversible state change is a collapse of the wave function.
6. Whether or not a collapse will occur cannot be determined in advance with certainty; only transition probabilities can be given.
7. Such collapses occur independently of whether the resulting state is observed or not, since no assumptions about information or observation are made.

The label 'the measurement problem' means quite different things in the epistemic and the ontic understanding of unsharp/ indeterminate values of observables. If we think that indeterminate values reflect epistemic limitations on our ability to gain knowledge through measurements, we need to know more about the details of the measurement process, viz., details that explain why precision is impossible. But if we think that indeterminate values reflect ontic facts about nature, i.e., that some observables do not have precise values before a measurement is performed, (i.e. denying value determinism) we may conclude that the physical interaction with the measurement device performs a discontinuous change of the state of the measured system, a change from not having a determinate value to having a determinate value. Such a state change is inevitable if interaction is quantised, since no mathematical representation of discontinuous state changes using continuous functions is possible. But we know by independent arguments that interactions occur in discrete steps, so, assuming the ontic view, we have a kind of explanation for the collapse.

The measurement problem arises when one tries to get rid of the full consequences of the quantization postulate and interpret quantum theory such that it fulfills the classical demands on a theory of the dynamics of physical systems, i.e., continuous evolution, determinism and reversibility. This is, I hope to have proven, impossible. If we accept quantization, we have to accept that there are real state changes that are irreducibly discontinuous, indeterministic and irreversible, i.e. there are collapses.

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